

Interactive comment on “Characteristics of an avalanche-feeding and partially debris-covered glacier and its response to atmospheric warming in Mt. Tomor, Tian Shan, China” by Puyu Wang et al.

Anonymous Referee #2

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Review of manuscript: “Characteristics of an avalanche-feeding and partially debris-covered glacier and its response to atmospheric warming in Mt. Tomor, Tien shan, China” by P. Wang, Z. Li and H. Li submitted to the Cryosphere

This paper presents a description of measurements of different type over one glacier in the Chinese Tian Shan (Qingbingtan glacier no. 72 in the Mt. Tomoro area). The measurements include: 1) mass balance observations in a section of the glacier tongue, starting from 2008; 2) GPS observations of surface velocities conducted once every year starting in 2008; 3) measurements of surface elevation obtained with the same

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GPS setup; 4) a topographic map from 1964 and a SPOT5 satellite image from 2003 used to assess changes in the glacier terminus position and in glacier area; 5) Ground Penetrating Radar (GPR) measurements of ice thickness at five transverse and four longitudinal transects; 6) measurements of ice temperature at three boreholes (two on clean ice and one at a debris-covered location) (four readings over two years); 7) manual measurements of debris thickness at various points; and 8) an Automatic Weather Station (AWS) installed on the glacier in 2008, complemented by three more after 2009.

This is a relatively abundant data set, spanning the period from 2008 until 2013, in a region where data are scarce, and it would be worth of publication. However, the paper has many serious flaws, and lacks a coherent structure and clearly defined goal beyond the description of the many data sets (this in itself lacking key details). The analysis and methods of data processing are poorly explained (see general comment below).

The conclusions and some of the authors' main results are speculative, and inferred from assumptions or values taken from literature somehow extrapolated to the study site. This is a major criticism to the paper. In particular, it regards: i) the assessment of the position of the ELA, which has a clear meaning and cannot be estimated from extrapolation of the linear variation above 4020m, obtained from very few points (not clear how many and which one because of the bad quality of Figure 5 and the associated description); ii) vague statements about the precipitation amount on the glacier which is determined “From meteorological observations in the ablation zone and the observation data from other glaciers in the same region ” (lines 348-350) – no details of either is provided and yet the authors come up with a number of 700 mm for the annual precipitation in the ablation area; iii) the same holds for the estimates of precipitation in the accumulation area, which the authors estimate from value from “an expedition to Mt Tomor in the 1970s” and considerations of a general increase in precipitation in the region (lines 345-360) which seem to allow the authors to derive a value of “no less than 1000 mm” for the accumulation area; iv) the corresponding estimates of total annual accu-

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mulation – which the authors come up with in a mysterious way. They state: “the total annual accumulation could be $4-10 \cdot 10^6 \text{ m}^3$ ” (my italic and bold); v) the entire section regarding ice temperature, all based on speculations with no support from data or any analysis: “Therefore, temperature at the glacier bottom must be at the melting point” (lines 455-457), or the following discussion on lines 458-464; vi) the entire reasoning on the behaviour of the debris cover portions of the glacier is also highly speculative (in particular from line 557 onwards until at least 569) and is based on general arguments existing in the literature on the general behaviour of debris covered glaciers that are then somehow bent to derive an assumed behaviour of the glacier studied here.

Similar vague statements that are not supported by any evidence or derived rigorously from any analysis can be found throughout the paper, including in the Conclusions (e.g. lines 583-593). This is an unacceptable approach to estimate values of interest or determine mass balance quantities, and should be corrected throughout the paper.

Statements are made throughout, but especially so in the Results and Discussion sections, which are made with no support, it is not clear if they are backed by the authors' evidence and results, or are common sense assumptions that the authors extrapolate from literature to then however infer future behaviour and characteristics of this specific glacier that are presented as findings of the paper. This needs to be thoroughly and carefully amended throughout the manuscript.

1) GENERAL COMMENTS

These are substantial comments that I would encourage the authors to follow.

1) ENGLISH and SCIENTIFIC WRITING The English is poor (both grammar and style) and needs to be substantially improved. I refrain here from making detailed suggestions because these concern the entire paper and the majority of sentences and paragraphs and would take a huge amount of time, but I suggest the authors ask for professional support.

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More importantly, the authors use often a colloquial language that is not appropriate for a scientific publication and should be removed and the paper style improved accordingly (e.g. glaciers that “inevitably” influence water resources, Introduction; “As we all know, climate is the essential factor determining glacier variation”, Discussion).

Even more importantly, however, the authors should change substantially the way they infer and then describe several of their major results and conclusions: these are too often only speculative, as I have discussed in details in my general evaluation above. All the instances detailed there should be addressed and corrected, and evidence provided for those statements and the corresponding so-called results changed or removed. The list above is not exhaustive and so the authors should carefully search their manuscript for other instances of the same way of writing and deducing results. This is a key comment that I encourage the authors to address.

In places, text that belongs to the Discussion is included in the Results (e.g. lines 260-268).

2) LACK of a CLEAR AIM and FOCUS The authors have a large amount of potentially interesting data but it is not clear what the main goal of this paper is. If it is to describe general changes of the glacier, some of the data are not well interpreted/exploited and I would recommend the authors from trying to establish the “glacier response to climate change” but only try to document as best as they can recent glacier variations. They should provide much sounder evidence for their analysis to back up their results.

3) METHODS Methods need in general to be substantially improved. The paper is poor in many respects, and important details are not provided. The location of the AWSs is not indicated in the map, nor is it clear from the paper if these were installed on or off glacier.

In general, one or more tables with the details of all measurements (setup, location, instruments, temporal resolution, etc) should be provided. As an example, we do not know where the AWSs were located, the location of the boreholes, etc.

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Other methodological aspects/sections that need to be improved include: - The kriging method used to interpolate the observations of ice thickness: no details about the method, which kriging approach is used (there are many: simple kriging, ordinary kriging, kriging with drift, etc), how the variogram was estimated, which theoretical variogram was fitted, etc.

- Uncertainty in glacier area and terminus change: the authors indicate a formula they use to calculate this, but it is not clear how they come up with the actual values from that formula "Integrated evaluation indicated that the resulting uncertainties... etc" (lines 192...). They should provide the values for each of the variables/terms in equation 3 and 4. In general, their methods should be reproducible, which is not the case at present for most of their approaches.

- Ice thickness estimation: I am not an expert in GPR, but there must be a more appropriate reference for the equation to estimate the ice thickness and associated uncertainty than a paper published in a Chinese journal. It would also be useful to associate an uncertainty to the ice thickness values estimated, or provide a sensitivity analysis. It is not clear where does Eq. 6 come from.

- Discussion of the character of the glacier (continental or maritime): no elevation is given for the boreholes, so comparison with temperature at other sites is not very meaningful.

4) Mass balance equation I am not sure that equation makes sense, with its three terms. What is the "affiliated" ice (line 132)? It is much more common to show the mass balance as the sum of its accumulation or ablation components. Even considering only the annual mass balance, it is not clear how the authors can identify the one of snow and ice (and of "affiliated" ice) at the same location from stake readings.

5) TREATMENT of DEBRIS COVER This also needs substantial improvements. First, a lot of text in the results belong to a general discussion on the topic and not actual results (e.g. lines 375-383), and should be either included in the Introduction or removed.

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Second, the authors do not map debris cover (either manually or from satellite images) and a map of debris cover should be provided in Figure 1. We see the possible location of debris from the map of reconstructed debris thickness in Figure 7.

Thirdly, and importantly, it is not clear how the authors come up with a critical thickness of 4cm (line 385). This should be justified in a convincing manner. The same is valid for the statements in lines 388-389, where the authors say that below 0.4-0.5 m the ice melting becomes negligible, but do not show any figure, data or evidence for this.

Fourth, it is not clear how they calculate the area of debris cover thicker than the critical thickness— indeed they never mention any estimate or calculation of the debris cover area before in the methods or data section (was it mapped manually, derived from satellite images?). Do they infer the area from the point measurements of thickness? This does not seem to be the case since the thickness point measurements are interpolated, so the area of the interpolation must be prescribed before. Details are needed here.

Fifth, and importantly, also this section is affected by the vague and speculative statements typical of the paper, with a lot of assumptions about what will happen to the glacier debris-covered area and to the melt that are not in the least supported (lines 392-400). The authors themselves admit they have no observations of what they are describing (line 394).

6) ABSTRACT The abstract needs to be entirely rewritten, both for English and content.

7) INTRODUCTION The Introduction needs to be rewritten. In its current form it does not provide a clear rationale for the paper nor states a well-defined goal, and importantly the literature cited is not appropriate. For example, the studies cited as references for differential response of glaciers do not seem nearly comprehensive enough.

8) RESULTS

Several statements are made in the paper's Results section but it is not clear where

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that evidence or specific results come from (see general comment above), a lot of it is speculative and this hinders an assessment of the soundness and validity of the authors' findings. - In general, Section on Changes in Glacier Mass balance and Volume (4.3) needs to be substantially improved, including the description of Figure 5 and the actual Figure 5 needs improvements: see points i) to v) in my general evaluation above and comment below about Figure 5.

The extrapolation of the thickness reduction to the entire area (lines 370 on) is questionable and I would remove this part, or justify it in a sounder way. As the authors themselves recognise, the results are very rough (line 372).

- Values of terminus and area changes in Table 1 should all be accompanied by uncertainty estimates (which in some cases the authors have derived and are provided in the text) to be able to make a meaningful comparison between the three periods (1964-2008; 2003-2008; 2008-2013) and across case studies.

9) FIGURES The quality of most figures needs to be improved, both graphically and in terms of content. - Figure 1: missing key locations of observations, e.g. the position of the AWSs. - Figure 2 needs to be improved, it is not clear if it is real or a scheme. There should be a background to the lines, be it the map or the SPOT image, or a DEM of the area. - Figure 4 should be replaced with a surface figure based on colours (also shades of grey are fine), not isograms. - Figure 5: the authors have to show the points here of their observations, not a continuous line that it is not clear how it was drawn. As it is, it seems that the authors have continuous observations in space

10) LITERATURE and REFERENCES Often the authors' literature is limited to Chinese papers (sometimes not available in English), also for issues for which there are ample literature in international, peer-reviewed journals. They should make an effort to expand that.

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